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Recent Developments in the SO(5) Theory of High T_c Superconductivity

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In this talk I outline the general strategy behind the $SO(5)$ theory of high T_c superconductivity. Progress in the direction of exact $SO(5)$ models, numerical exact diagonalization and possible experimental tests are reviewed. I also address the criticisms raised recently against the $SO(5)$ theory and point out directions for future exploration.

In this brief review I would like to summarize the recent developments in the $SO(5)$ theory of high T_c superconductivity [1,2]. Since my review in the Proceedings of the International Conference on Materials and Mechanisms of Superconductivity [3], some important progress has been made due to the efforts of many groups [4–12]. There has also been the criticisms raised. I shall start by briefly outline the general idea and strategy behind this theory, then proceed to discuss recent progress made in constructing exact $SO(5)$ symmetrical models, evidence from exact numerical diagonalization of the $t-J$ model (see Prof. Werner Hanke's contribution to this Proceeding for more details), and various proposals for experimental tests (see Prof. John Berlinsky's contribution to this Proceeding for more details). I shall address some criticisms raised recently by Greiter [13,14], Baskaran and Anderson [15], both from model dependent and general considerations.

In many people's mind, the theoretical problem of high T_c is to start from the "realistic" model Hamiltonian, reduce it to a simple form and solve it to see if it produces the generic phase diagram of the high T_c superconductors. Ten years after the discovery of high T_c superconductivity, it becomes clear that this is strategy does not work very well. There are no controlled ways of reducing a "realistic" Hamiltonian to simpler forms and even if one accepts simple models like the Hubbard or the $t-J$ model, one can still not determine its phase diagram. In view of these difficulties, we would like to propose a different strategy, essentially by reversing the arrow of the above mentioned logic. *First let us start with the generic phase diagram of the cuperates, assuming that antiferromagnetism (AF) and d-wave superconducting (dSC) order are the only zero temperature phases in the clean limit, and ask what kind of Hamiltonians can give this type of phase diagram.* If we only require the model to have SC order in its ground state, then the problem is trivial, since any model with purely attractive interaction will surely do. However, such models will in general not have AF order at half-filling. Models with AF order generally require repulsive interaction, and a generic model will not give SC order away from half-filling. Therefore, the first problem we opposed is highly non-trivial, but well-defined mathematically. Supposed that this problem is solved, *our next goal is to show that the special model Hamiltonians we found are adiabatically connected to the real system.* This goal is less well defined, but can still be investigated with a reasonable degree of satisfaction. Because the AF and SC phases are infrared fixed points in the RG sense and thus stable at zero temperature, models which can produce both of them have a good chance of being adiabatically connected. One can also investigate the special models and "realistic" models numerically, and try to follow the low energy levels as best as one can. Having established the first two goals, *our final objective is to formulate a general theory with a small number of phenomenological constants, which captures all the qualitative physics of the special models and can be fitted quantitatively to experiments.*

Notice that the above strategy is formulated in exact footsteps after the strategy behind the Landau fermi liquid theory. I mention this theory only in the context of its strategy, and I am not saying that it is applicable to the normal state of high T_c materials. In the Landau fermi liquid theory, the goal is to understand real systems like metals, normal ^3H_e liquids, heavy fermion compounds etc. The first goal is established by the free fermi gas model, which is exactly solvable and is the only concrete example of a fermi liquid. The second goal is established by Landau's celebrated adiabatic hypothesis. This hypothesis has never been proven but is extremely powerful once it is accepted. It makes the free fermi gas model a prototype model for a wide class of systems which are far from it in Hamiltonian space. The last objective is of course achieved by Landau's phenomenological fermi liquid model which can offer quantitative analysis for many experiments.

Given the historical success of Landau's fermi liquid theory, it appears that the above formulated goals towards the high T_c problem may indeed be realistic. How can we then achieve the first step? This was a trivial step in the fermi liquid theory but extremely difficult in the present context, for above mentioned reasons. Since this is no trivial matter, we need a principle, rather than a trick, to solve the problem. In order to find models which can give *both* AF and dSC order, it is natural to build in the symmetry between these two phases. This is precisely the idea behind the $SO(5)$ theory. $SO(5)$ symmetry is a symmetry between the AF and dSC phases, therefore, if models with exact $SO(5)$ symmetry have AF order, it must also have dSC order. At this point, three independent groups have constructed such *microscopic* $SO(5)$ models [8–10], and reference [8] gave a general group theoretical classification for this class of models.

Some of these models may have neither AF nor dSC order, some of them have both, but it is not possible to have one without the other. At half filling, the basic reasons for the Hubbard model to have AF order also applies to a large class of these models. Therefore, by the principle of $SO(5)$ symmetry, we can state with the same degree of confidence that *we now have microscopic models which have both AF order at half-filling and dSC order away from half-filling.* Important progress has also been made in the second stated goal. Exact diagonalization studies on Hubbard and $t-J$ clusters [4,12] by Hanke's group show striking evidence that the low energy states agree with the level structure anticipated from $SO(5)$ symmetry, and therefore gives some indication that the microscopic models with exact $SO(5)$ symmetry can be adiabatically connected to more "realistic" models. Progress towards the final goal is made historically before the first two. The $SO(5)$ quantum rotor model with anisotropic couplings captures the basic physics of the microscopic $SO(5)$ models, but contain phenomenological parameters which may eventually be determined experimentally [6,11]. It is tempting to believe that all models with this type of phase diagram flow to this theory in the long wave length limit [16].

In the problem of high T_c superconductivity, we are not dealing with a new state of matter. Both the AF and dSC phases are well-characterized by their respective broken symmetries. However, while these two basic phases are known and well-understood, their intimate relationship is the key challenge in the high T_c problem. Never before in condensed matter physics have we encounter the competition between two diagonally opposite types of broken symmetries — namely the insulating diagonal-long-range-order (DLRO) on one side and the superconducting off-diagonal-long-range-order (ODLRO) on the other — manifested on such a grand scale. Because our understanding of their competition and possible unification is of such a paramount importance to condensed matter physics in general, one should be given the license strip away all the model dependent details and extract general principles which captures the core physics unifying AF and dSC. Unification of fundamentally different *forces* by symmetry principle is a central scheme of the 20th century particle physics. The idea behind the $SO(5)$ theory is that fundamentally different *phases* in condensed matter physics can also be unified by similar symmetry principles.

After outlining the general strategy, let me now turn to review the details of the recent progress. Let me first start with the microscopic $SO(5)$ models. This problem was solved by three independent groups [8–10]. Two technical innovations helped this progress. One is the Henely-Kohno factor $\mathrm{sgn}(\cos p_x - \cos p_y)$, introduced by Chris Henley and Hiroshi Kohno independently. This factor has the symmetry property of $d-$wave, and its square is unity. If one uses this factor in the definitions of the π operators, rather than the conventional $\cos p_x - \cos p_y$ factor used by Demler and myself [2,1], the $SO(5)$ algebra closes exactly. The second one is the concept of a spinor [8] which transforms according to the irreducible representation of $SO(5)$. Therefore, one can construct general exact $SO(5)$ symmetric models simply by enumerating the possible bilinears that can be constructed from this spinor. The result is a surprisingly large parameter space (or actually functions), which support exact $SO(5)$ symmetry. These models have the following attractive properties: 1) One can prove that if the model has AF order at half-filling, it must have dSC order away from half-filling. This fact follows from the $SO(5)$ symmetry and the fact that the chemical potential term commutes with the Casimir operator of $SO(5)$. 2) The π operators are *exact* eigen-operators of the Hamiltonian, and the π resonance below T_c predicted by Demler and myself [2] is a *exact* excitation of the system. It has exactly the same quantum numbers, the energy and intensity dependence on doping as the experimentally observed resonant neutron scattering peaks in $YBCO$. 3) The SDW quasi-particles are related to the BCS quasi-particles by exact $SO(5)$ rotations and the AF/dSC transition is simply a "gap rotation" transition, rather than a "gap closing" transition. This behavior may offer a basic explanation of the pseudo-gap behavior observed in the underdoped cuperates, and can be directly tested experimentally by construction suitable AF/dSC junctions [5,11] and by studying the nature of the fermionic excitations at the boundary. In fact, the $SO(5)$ generalized Bogoliubov-de Gennes equations should be intensively studied.

Important progress has also been made in our second stated goal. Since the microscopic $SO(5)$ models have the same phases as the real system, it is quite plausible that they are in fact adiabatically connected. (It certainly is more plausible than the fermi liquid hypothesis since broken symmetry phases are much more robust than the fermi liquid states). However, it is not so clear if they are actually adiabatically connected to the models of the real system, since their phase diagrams have not yet been established. We can try to establish the connection by testing the low lying energy level structures of the Hubbard or the $t-J$ model *numerically* [4,8]. The $SO(5)$ symmetry predicts a well-defined structure of the low energy states. For example, one can start with the one magnon state at half-filling and analytically construct a $SO(5)$ rotated state by acting the π-annihilation operator on it. This state has two holes and one can ask about its overlap with the ground state in the two hole sector. If the ideas of $SO(5)$ work, this overlap should remain finite in the infinite system size limit. Numerical results finds clearly identifiable spectral peaks where these states should overlap. This process can be carried to higher doping, and it continue to work until doping concentration exceeding 25%. The energy splitting between the one magnon state at half-filling and the two

hole ground state is a important parameter. It certainly depends on the chemical potential since these two states have different electron number. However, once this number is fixed, it should be the same number that determine splitting of other states connected by the π operator, e.g. the two magnon states, the triplet 2 hole state and the 4 hole ground state etc. Therefore, the variance of the splitting among various states connected by the π operator is a well-defined numerical measure of how good is the π operator a eigen-operator of the $t-J$ model. For the $J/t = 0.5$ model, this variance is about $0.07t$, much smaller compared to J.

These numerical data are obtained from finite sized systems, and one should always be careful with their interpretations. Usually, the energies do not change significantly with system size, but the overlaps do. It is therefore important to check the size dependence of the overlaps. But the near equality of the $SO(5)$ multiplet levels at the critical chemical potntial shed significant light on the nature of the transition between AF and dSC. Generically, the transition from AF to dSC is expected to be first order, which could terminate at a critical point. But there is always a question on whether it is more Ising like with disjoint configurations at both sides of the transition, or more like a Heisenberg-Neel model in a field, where the order parameter rotates through a continuously connected set of states with small energy barriers. From the $SO(5)$ multiplet structure, we can see how the superspin vector is rotated from AF to dSC direction, and identify the intermediate states which connect them continuously and show that at the critical chemical potential, the energy barrier between AF and dSC is smaller than the natural parameters in the model, namely t or J. This finding is extremely important, and I believe that it will not change significantly with system size. It gives us confidence that the first order transition between AF and dSC is more Heisenberg like rather than Ising like and is accompanied by the soft collective excitations. These soft collective excitations may make the dominant thermodynamic contributions in the underdoped regime and may be responsible for the pseudogap physics.

Clearly, the microscopic $SO(5)$ models are only *prototype models* for illustrating general principles and qualitative features, not to be used for quantitative comparisons with experiments. On the other hand, the phenomenological $SO(5)$ quantum rotor model [1,3] captures the basic physics, offers a simple and intuitive picture of the AF/dSC transition in analogy with spin flop transition, and can in principle be used for quantitative comparison with experiments and possibly predicting new effects. The $SO(5)$ quantum rotor model is a bit like a Landau-Ginzburg theory for the high T_c problem. However, it goes much beyond the traditional LG theory since it contains *quantum dynamics*. The validity of this model to the real system rests on the principle of adiabatic continuity. It can contain large $SO(5)$ anisotropies, as long as the various collective modes remain the lowest energy excitations within their respective quantum number sectors. Recent developments in this area are the following: 1) Using this type of models, it was predicted that a superconducting vortex in the underdoped materials has a AF rather than normal core [6]. 2) It was used to explain the puzzling long ranged proximity effect in Pr doped $YBCO$ superconductors, and a novel transition in the SC/AF/SC is predicted as a function of the AF layer thickness or the applied current [11]. 3) The stripe phase observed in some high T_c materials may actually be a "$SO(5)$ superspin spiral" [17]. In this configuration, the superspin vector points in the AF direction on a 2-legged ladder, in the dSC direction on the next 2-legged ladder, in the π phases shifted AF direction on the next 2-legged ladder, and finally, in the π phase shifted dSC direction on the last 2-legged ladder, before the structure repeats itself. The last statement is a new prediction. Therefore, travelling in the direction transverse to the stripes, the $SO(5)$ superspin spirals on a great circle. Prof. John Berlinsky will summarize the details of these works in his contribution to the proceeding.

Recently, Greiter [13], Baskaran and Anderson [15] raised criticism against the $SO(5)$ theory. Part of their comments are model dependent questions concerning the applicability of the $SO(5)$ model to realistic systems and part Baskaran and Anderson's comment challenges the core concept and the overall direction of the $SO(5)$ approach. I shall briefly address the model dependent part first, more details will be given by Prof. Hanke in his contribution. Greiter argues that the energy of the π resonance *in the metallic phase* is of the order of U, not J. This is clearly incorrect. The source of the error has been traced in weak coupling [14], where his initial arguments was based. Here we present the strong coupling version of the argument. The π operator is a spin triplet operator, therefore, the mutual interaction among the two electrons inside the pair can only be of the order of J. However, when the π pair is injected into a metallic (or superconducting) state, its energy can be of the order of J, U or $2U$, depending on whether the π pair goes into two empty sites, one empty and one singly occupied sites or two singly occupied sites. Therefore, the π spectra should have three peak structure, with the two high energy peaks smeared into bands due to scattering. As long as the system is less than half-filled, the low energy peak will have a finite spectral weight, proportional to doping x. Numerical results on the Hubbard model clearly demonstrate this peak structure and showing that *the low energy peak scales inversely with* U. For low energy physics, we are only interested in the lowest peak. The difference between the spectral distribution and the average spectral energy could be the source of the confusion. While Greiter's arguments are incorrect for the metallic or the superconducting state, they are applicable to the *insulating state* where the chemical potential is discontin-

uous, and there are no empty sites. The implication of the chemical potential discontinuity was already worked out in reference [1]. The π doublet excitation of the insulator splits in energy when the chemical potential is varied from the center of the gap. At the critical chemical potential μ_c, the energy of the π^- mode vanishes and that of the π^+ mode remains finite. This energy difference reflects the discontinuity of the chemical potential at half-filling.

The beginning part of Baskaran and Anderson's comment addresses the stability of the π mode against possible perturbations. The effect of the next nearst neighbor t' is a important issue and is not fully understood analytically, but numerical calculations do show that the π mode is stable against t'. The problem is that one obtains a t' contribution from band calculations which fits the shape of the $YBCO$ fermi surface but grossly overestimates the band dispersion around the $(\pi, 0)$ and $(0, \pi)$ points by at least one order of magnitude. Both photoemission experiments and numerical diagonalizations show that the band dispersion around these points is less than $10meV$. The π mode is essentially formed by the multiple scattering around these points and it can only be stable against t' if the band dispersion at these points are small. It is very hard for current analytical calculations to produce this band narrowing effect, since it is of a subtle many-body origin, and check the stability of the π mode. However, numerical calculations on the Hubbard and $t-J$ model show that the π resonance is actually stable against t'. The next question raised by Baskaran and Anderson concerns the effect of V, the next nearst neighbor interaction. Their question about the $-1/4n_in_j$ term in the J part of the Hamiltonian is mathematically identical to the question of a general V term. Unlike the Hubbard interaction which only acts on the same site and has no effect on the mutual interaction within the π pair, the V term certainly shifts the energy of the π peak. However, it also shifts the energy of a $d-$ wave hole pair, in very much the same way. Therefore, one would expect on general grounds that V does not affect the difference between the energy of a π pair and a $d-$ wave hole pair. Numerically, this indeed seems to be the case. In the actual experiment, the observed neutron resonance corresponds to a process where a $d-$ wave hole pair is extracted and a π pair inserted. *The resonant energy therefore measures the difference in their energies.* Computer calculations can miss small energy differences due to finite size effects, but a effect on the order of eV, as argued by Baskaran and Anderson, can certainly be distinguished.

These types of model dependent debates are important at a later stage of the development in any theories on high T_c, when one compares quantitative predictions with experiments. At the current early stage of the $SO(5)$ theory, it is much more important to make sure that the basic core ideas are correct and not in conflict with well-established general principles. That is why the general criticism raised by Baskaran and Anderson should be the main focus of the debate. *Neither the $SO(5)$ theory nor the RVB theory are in conflict with any fundamental principles of physics, the difference between them lies in the strategies of attacking the high T_c problem.* In their comment, Baskaran and Anderson quoted the "Elitzur's theorem", and gave the impression that it is a fundamental and rigorous result which is in conflict with the basis of the $SO(5)$ theory. In fact it is a result that has no direct applicability to most condensed matter physics models in consideration. The "Elitzur's theorem" states that local gauge symmetry can not be broken spontaneously. In condensed matter physics, the only local gauge symmetry is the freedom of choosing the phase of a wave function locally, at the expense of a gauge transformation on the electromagnetic vector potential. The consequence of the Elitzur's theorem is to give the phase mode of a superconductor a finite mass, which is the well-known Anderson-Higgs mechanism. In any model without the real electromagnetic fields, like the Hubbard or the $t-J$ model, there is no *physical* local gauge symmetry at all. However, local gauge symmetry can be introduced artificially if one enlarge the Hilbert space artificially, for example by representing the Heisenberg spin operator in terms of bosons or fermions. In this case, the manifestation of the "Elitzur's theorem" is nothing but the projection back into the actual physical space itself! This kind of artificial enlargement of the Hilbert space maybe useful in some approximate mean field theory approaches, but are not suitable in discussing matters of fundamental principles.

Therefore, the true debate should be focused on the overall strategy and philosophy towards attacking the high T_c problem. The basic difference lies in the starting points. From the insulating side, is it better to start from a AF state or a Mott insulator? From the metallic side, is it better to start from the actual SC ground state or a "Luttinger liquid" state? The first question of course is only well-defined if the Mott insulator state is a RVB spin liquid state without any long range order. However, soon after the discovery of high T_c superconductivity, it was established both experimentally and theoretically that the Heisenberg model on a square lattice does have long range order. On frustrated lattices, spin Hamiltonians either find other types of order or go into spin-Peirls states. It appears that the RVB type of order could only occur at the quantum critical points between these various types of order, and it may not exist as a phase in two or higher dimensions. Sometimes, the dichotomy at half-filling is formulated by contrasting the spin-density-wave (SDW) picture to the Heisenberg-Neel picutre of the AF state. There is however no qualitative difference between these two, since they are characterized by the same type of broken symmetry. Furthermore, Schrieffer, Wen and I [18] have shown that these two extremes can

be smoothly connected *quantitatively*, and the SDW approach can capture the physics at strong coupling as well. From the point of view of adiabatic continuity and symmetries, it is perfectly OK to think of the AF state as the result of a fermi surface instability, in the same way that the SC state is the result of another type of fermi surface instability. The difference in the size of their gaps is only a quantitative matter. The dichotomy between the AF state and the RVB state at half-filling is translated into the dichotomy between the dSC state and the "Luttinger liquid" state away from half-filling. Once again, systematic calculations failed to show Luttinger liquid behavior in two dimensions. The only instability of the fermi liquid away from commensurate fillings appear to be SC order. Experimentally, the zero temperature state at optimal doping is a dSC state rather than a Luttinger liquid state. Therefore, like the RVB state, the Luttinger liquid may not be a phase but only a quantum critical point between phases.

Laughlin [20] has recently proposed that the main conceptual differences between the RVB type and the $SO(5)$ type of theories can be removed if one accepts the idea that RVB, Luttinger liquid states and spin-charge separation only exist as isolated points in the Hamiltonian space in two dimensions, therefore, it may not be in conflict with the general ideas of a quantum critical point [19] and also not in conflict with the $SO(5)$ idea in particular, since it is well-known that *extra symmetry can be present at critical points*. These points only exist because of the competition and unification between various types of order. What are the most important and robust types of order in the high T_c problem? Obviously they are the AF and dSC order. From this point of view, RVB and $SO(5)$ theories are actually addressing the same type of physics. Laughlin's insight not only offers a possible unification of two seeming divergent theoretical ideas, but may also lead to practical progress by complimenting the ordering physics near the AF/dSC transition with the novel transport physics in the high T_c superconductors.

In conclusion I summarized here recent results in the $SO(5)$ theory of high T_c superconductivity. Rapid progress has been made towards the three stated goals of this approach. The model depedent part of the criticism has been answered both by general arguments and detailed numerical calculations. The difference between the $SO(5)$ theory and the RVB theory mainly lies in the overall strategy of attacking the high T_c problem, and only time could tell which one would work better. Major direction for future theoretical development should concentrate on the possibility of zero temperature $SO(5)$ symmetric quantum critical point, more quantitative numerical tests of the $SO(5)$ symmetry in known models, working out quantitative predictions from the $SO(5)$ quantum rotor model and studying the nature of the fermionic excitations in the AF/dSC transition region. In comparison with experiments, it is most important to establish the high T_c phase diagram in the AF/dSC transition regime. $SO(5)$ theory is a rather bold hypothesis, and many striking qualitative experimental predictions remain to be worked out. The strange and mysterious quantum mechanical world where DLRO and ODLRO are unified must have profound experimental manifestations filled surprises and puzzles.

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